Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2017-201-RC1, 2017

Natural Hazards and Earth System Sciences
Open Access
Discussions

Interactive comment on "Brief Communication: 3D landslide motion from cross correlation of UAV-derived morphological attributes" *by* Maria V. Peppa et al.

Anonymous Referee #1

Received and published: 31 August 2017

The manuscript presents an interesting application of UAV products for remote monitoring of landslide motion. In particular, the Authors calculated DEM and derived morphological attritutes such as slope, curvature, shaded relief and opennes at six different times. Differences of DEM and morphological attributes were calculated and utilized as inputs in an existing software package to calculate cross-correlations. The procedure adopted in this work was first applied to a synthetic displacement in two test areas, simulating the motion of one real landslide, to calibrate various software parameters. Eventually, the procedure is validated against ground truth measured independently in the field. The validation procedure is apparently essential to obtain the final results: a limitation which is actually acknowledged by the Authors.

The novelty of the approach is represented by the use of many morphometric attributes, at variance with previous approaches using the sole shaded relief. The calculation of DEM and morphological attributes from UAV imagery allows fast, cheap and, most importantly, repeated in time measurements, a fundamental requirement for monitoring slow landslides.

In my opinion, the Manuscript suffers from two limitations. Though the presentation is generally clear, a few details of the method are not. Also, the conclusions drawn by the Authors are somewhat incomplete and fail to report the major criticality of the method, namely the need of calibration/validation field data.

As a matter of fact, the use of morphological attributes, their signal-to-noise ratio, the optimal choice of the best attribute and reasons for that, the possibility of describing different details of the landslide body with different thresholds for openness are described at length, while the use of DEM differencing is barely mentioned in a few points throughout the paper. This does not appear enough to understand how the technique was useful to obtain the results. Both the Abstract and the Conclusions section explicitly mention that "the analysis has illustrated that the fusion of openness morphological attribute along with DEM differencing can support the comprehensive interpretation of landslide behaviour", implying that the technique was equally important than the morphological analysis thus the Authors should describe how they performed the "fusion".

Another unclear point, though less important, is the description of manual removal of spurious displacement vectors. How did the Authors distinguish such vectors? Is the sentence at lines 27-29, page 6, about rotational features enough to describe that, and is the knowledge of presence of rotational (local?) failures a further requirement for the successful application of the procedure? Is it so straightworward to define proper thresholds to automatically remove the spurious vectors, or does it require further calibration, thus relying even more on field data?

I also believe that the lines 21-31, page 4, and 1-2, page 5, in the Results section,

belong to the Method section. In fact, they characterize the "calibration" step, so that they are not results of the method, strictly speaking.

In line 22, page 6, it was suggested that "winter would constitute the best period to conduct UAV surveys", due to presence of vegetation constituting a major noise source. Is it possible to deduce this statement quantitatively also from Fig. 2?

The Conclusions section, as noted before, should emphasize clearly that the method cannot be applied without field data. This is more of a Conclusions than proposing future work, for which the Authors spent one half of the Conclusions section, though necessarily it somehow reduces the relevance of the proposed method.

Figures:

in Fig. 3, it is not clear, at first glance, that the "Surface displacements", "d(D)isplacement sample points" and "SNR > 0.7" and "Elevation differences" legends apply to all the whole figure. Maybe a single legend would be more direct.

in Fig. 4c, maybe a legend would be better than the labels with arrows.

In summary, I believe that the Manuscript should be revised before it can be published as a regular paper on NHESS.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2017-201, 2017.